On Magnetic Disturbances, by Prof. W. G. Adams, F.R.S.—The author, in considering magnetic disturbances, stated that certain facts about them had long been known; from the observations of Gauss in 1834 the disturbing power was found to increase in northern latitudes; it was also found that the appearance of a disturbance occurred in several places at the same instant, but with great differences of results. The force seemed to originate at a certain point in the interior of the earth, and the direction of the disturbing force seemed constant, yet great differences were observable at places not remote from one another. Sabine found that these disturbances had daily and yearly variations from their mean values, and that they have an eleven-year period corresponding to the appearance of spots upon the sun. It has been shown by observations that magnetic disturbances and electric currents on the earth are related; these electric currents in the earth have commonly been attributed to changes of temperature. The month of March, 1879, was chosen for a comparison of the photographic records of magnetic disturbances, and records for the whole month were sent from Lisbon, Coimbra, Stonyhurst, Vienna, St. Petersburg, and Bombay in the northern hemisphere, and from Melbourne and the Mauritius in the southern hemisphere. Taking the disturbances on March 15–16, 1879, as an instance, we see that soon after 10 a.m. Greenwich time on the 15th, a disturbance-wave happens, which shows first a diminution and then an increase of horizontal force at St. Petersburg, Vienna, Kew, and Lisbon, and also at Melbourne in Australia. At 9.30 p.m. of the same day a magnetic storm begins, and continues for about an hour. It is felt in the northern and southern hemispheres. At all stations in Europe the horizontal force is increased in the first part of the storm, and then diminished. At Lisbon the vertical force is first increased and then diminished, and at St. Petersburg and Stonyhurst there is a diminution in the vertical force at the same time as at Lisbon. Regarding the declination needles, we find that at St. Petersburg, Melbourne, and Bombay the declination westward is first increased and then diminished, whereas at Kew and Lisbon the motions are in opposite directions. At Bombay and Mauritius, near to, but on opposite sides of, the equator, the declination needles are deflected opposite ways. If we assume that by magnetic induction the earth's magnetism is altered, the position of the magnet which would cause the disturbance must be such that its pole, which attracts the marked end of our needle, must lie at the beginning of the disturbance to the east of Kew and Lisbon, to the north of Vienna, and to the north-west of St. Petersburg; the Lisbon vertical force curve also shows it to be below the surface of the earth. Hence an inductive action equivalent to a change of position of the north magnetic pole towards the geographical pole would account for these changes. The strengthening and weakening of a magnet with its north pole to the north on the meridian of Vienna might account for magnetic changes observed between 9.30 and 10.30 at night, Greenwich time, on March 15, 1879. In attempting to explain this disturbance by currents of electricity or discharges of statical electricity in the air above the needles, we must imagine that at first there is a strong current from the south-west over St. Petersburg, from the west over Vienna, and from the north-west over Kew and Lisbon, the vertical force needle at Lisbon showing that the current from the north-west lies somewhat to the east of Lisbon; that at the Mauritius this current is from the north, and at Bombay from the south. Thus we must imagine that a current of electricity passes down from the north-west to the south-east, going on towards the east over Vienna, and towards the north-east over St. Petersburg. This must be kept up very much along the same line throughout the first part of the disturbance, and then the current must be altered in strength in the same manner at all stations. An examination of the principal disturbances at Kew and at St. Petersburg seems to show that (1) a diminution in the horizontal force is accompanied by greater easterly deflections of the declination needle at St. Petersburg than at Kew; (2) increase of horizontal force is accompanied by greater westerly deflections at St. Petersburg than at Kew, or is sometimes accompanied by a westerly deflection at St. Petersburg and an easterly deflection at Kew. Only moderate disturbances have already been considered, and the author now treats of a much larger magnetic storm which began at 10.20 a.m. Greenwich time on August 11. This storm may be divided into three storms: one lasting from 10.20 on the 11th to 1 a.m. on the 12th; a second from 11.30 a.m. on the 12th to 7.20 a.m. on the 13th; and the third from 11.50 a.m. on the 13th to 7 to 8 a.m. on the 14th of August. The first storm began on August 11, at the same instant at all stations. There is a decided similarity, especially in the horizontal force curves, throughout the first part of this storm, and certain points in it stand out prominently. The deflections are alike at Lisbon, Kew, Vienna, St. Petersburg, and after the first very sudden deflection at Toronto also. The greatest effect is produced at St. Petersburg; the similarity between the large disturbances at Vienna and Toronto, in Canada, places differing about six and a half hours in time, is remarkable. About 11.45 p.m. and 2.40 p.m. there are very remarkable points of agreement. From about 4.30 p.m. to 8 p.m. Greenwich time, *i.e.* from about 11 a.m to 2.30 p.m. Toronto time, the deflections are opposed at Toronto and at Vienna or Kew. This would point rather to solar action as the cause of the disturbance. At 9 p.m. the disturbances are all in the same direction, but about 11 p.m., whilst St. Petersburg agrees in direction with the others in a very violent phase of the storm, at Toronto the direction of the deflections is reversed, and this reversal of curves continues until about the end of the first of the three storms. The second storm, the most remarkable of the three, began about 11.30 a.m. on the 12th, and lasted until the next morning. At Toronto the line goes off the edge of the paper on which the photographic record is taken. At Vienna and Melbourne the motion is so rapid that the plate is not sensitive enough to receive the impressions. At 12.20 midday, the time of greatest disturbance at Lisbon and at Zi-ka-Wei near Shanghai in China, two places nine hours different and nearly in the same latitude, the vertical force is increased in precisely the same fashion. At St. Petersburg the change in the horizontal force was one thirty-fifth part of the whole horizontal force, and the total force was changed to about one-eightieth part of its full value. These magnetic changes are so large as to be quite comparable, as we see, with the earth's total force, so that any cause which is shown to be incompetent from the nature of things to produce the one can hardly be held to account for the other.

The number of mathematicians who attended the meeting was very remarkable, and among the foreigners present may be mentioned Messrs. Halphen, Chemin, Rudolf Sturm, Cyparissos Stephanos, and W. Woolsey Johnson (Annapolis, U.S.). A separate mathematical department was formed, which met on three days, and more than thirty papers on pure mathematical subjects were read, many of them being of great interest. Prof. Halphen made a communication on Steiner's theorem relative to the positions of the centres of conics passing through three given points, and gave an elegant extension of the theorem to distinguish the cases in which the three points lay on the same or opposite branches of the curve. He also made communications on the subject of linear differential equations and hypergeometrical series; and in a fourth paper he considered the number of aspects in which points in a plane may be viewed. He showed that two points may be thus viewed in six ways, that four points can be viewed in nine ways, and illustrated this by a diagram, and extended the theorem to five points. Prof. Sturm communicated an elaborate memoir on curves of double curvature, relating to the researches of Cayley and Halphen, which was ordered to be printed *in extenso* among the reports. M. Stephanos read several papers, in one of which he showed that the different homographies which exist upon a straight line, and which are triply infinite in number, may be identified with the points of space. A simple and beautiful representation of the particulars of these systems was thus obtained.

The other papers included communications by Prof. Cayley, *On the Transformation of Elliptic Functions, and on Abel's Theorem*; by Prof. H. J. S. Smith, *On the Differential Equations satisfied by the Modular Equations, and on the Theory of the Multiplier in the Transformation of Elliptic Functions*; by Mr. J. W. L. Glaisher, *On the q-Series in Elliptic Functions*; by Dr. Hirst, *On Consequences of the Second Order and Second Class*; and by Prof. R. S. Ball, *On the Application of non-Euclidean Space to a Problem in Kinematics, and an Extension of the Theory of Screws to the Dynamics of any Material System.*

SECTION B—CHEMICAL SCIENCE

The Present State of Chemical Nomenclature, by Prof. A. W. Williamson, Ph.D., F.R.S.—The author stated there were perhaps few departments of science in which such definite principles had been adopted, and to a great extent this applied to the

formation of names, as in their own science of chemistry. The practice of stating in a name as briefly as possible certain facts, and as a rule important facts, had been, as every chemist knew, the chief object of their nomenclature. But he thought he might be permitted to say that if one looked to the composition of any result like the present nomenclature of chemistry—which had been guided by intellectual principles—it was of immense importance to consider its purely intellectual principles, viz., the principles of convenience, and perhaps even of popular tastes, and, if he might be allowed to imagine such a thing, even the prejudices which occasionally arose among a great number of men who adopted any particular form of expression. He proposed to refer to the question from the different points of view. If they had occasion to consider, without knowing anything about it, what was the most important condition to which every name ought to conform, he fancied there would be no two opinions on the matter. The first and most important condition and requirement of every name of a thing that was important was that it should call to the minds of those who used it, without ambiguity, some one particular thing or one particular idea. He should be inclined to consider a code of laws by which their action would be rendered uniform with regard to names, and which would establish such fundamental principles that an absence of ambiguity would be secured. The more any name could be defined and shortened the better it would be for chemistry. In the modern progress of chemistry, especially in that department of which the growth had been enormously great—he meant the many carbon compounds—the purpose of obtaining clearness and avoiding ambiguity in the nomenclature had been, with few exceptions, satisfactorily attained. But he thought members would agree with him that in the names given to some compounds more complex than others the chief object of convenience had not been attained to an equal extent. They found names given which, when carefully considered by chemists, told a story, but a very long story, and in a manner which was really free from ambiguity, but only by aid of a great number of syllables, and a compound word of inconvenient length was this attained. On the other hand, amongst very common substances that systematic process had been, he thought, to a considerably less degree adopted. The older names of commoner substances, such as salts, were to a great extent based upon facts which were true, but were by no means the only facts to be recalled. Of course every chemist knew the great number of names that were in common use, and how far they served to recall a particular process, but only one among many processes by which the substance could be formed. On the other hand, many names had grown up from bodies which were purely empirical—names which did not recall any particular properties, but served with great convenience and without ambiguity to indicate the body. If they looked to the circumstances which affected that one condition which he had submitted as essential to names being perfectly free from ambiguity, there was perhaps hardly one condition more practically important than this, that there should be in the names as little change as possible, and more especially was this the case when a name that had once been given had come to be used in relation to particular substances. It was within the memory of chemists that changes of name had taken place not only when a particular substance was recalled, but there were also a considerable number of cases showing that the name given at one time to one body was afterwards given to another. The circumstances attending such changes were in some instances of an exceedingly reasonable kind, and well worthy of consideration after it was found that there were grounds for believing that the names belonged more properly to other substances. If, however, changes introduced confusion, they were necessarily injurious to the progress of the science. When he looked back to the successive steps by which their knowledge had risen to its present position, and to the ideas that had succeeded one another, he felt that in order to really understand chemistry, and to be able to arrange the facts in a convenient order, they must see how they had grown up. If that was important in practical matters, it was even more important in what he might call the scientific work. He ventured to think, at all events he had always felt, that to use with safety any idea that they were accustomed to use, it was almost essential, and was certainly of importance, that they should endeavour to trace the origin and growth of that idea, so as to see what it really meant. His object in bringing the subject before the Section was to obtain from his colleagues and friends their views on the present state of matters, and to give them the opportunity of considering together—those who more especially felt it their duty to contribute by any means in their power to the advancement of science either in guiding the operations or growth of those names—whether there could not be greater concert among chemists as to what was being and what had been done, so that they might conform their doings to certain laws. He had frequently seen with regret some features in chemical nomenclature that had been springing up of late years. He had seen some habits gaining ground which appeared to be at variance with the best principles of nomenclature—he would assume such to be the case. But there were laws in the growth of those words, and he could not doubt for his own part that if chemists came to recognise those laws, or rid themselves of them, the future growth of words would gradually come to be a more systematic guide. It had sometimes been felt that to attempt to solve the problem would be useless, and that irregularities had become so prevalent that it would be hopeless to think they could ever remedy them. But he thought differently, and would urge that in the direction he had pointed out they were only now beginning to move. There was only one convenient division among names. That division, of course, was not absolute, because no such division could be absolute; but the great majority of names were used to denote things and ideas. Some names were of little use in relation to the particular ideas, and therefore it seemed to him that the best way to obtain a name was as the result of experiment. If founded on that principle there could be no ambiguity. At the present time, as their views had considerably changed, and as they had not attained finality in their operations, there was much to be learnt, and it was reasonable to suppose that if they adopted a particular name to indicate a particular thing it might perhaps turn out at some time hence an error upon which people would look back as historical. With regard to names, especially theories, there were some of them that had certainly served important purposes. It was then really essential to the arrangement of their ideas that they should for the time imagine something to exist, and that they should recall by some convenient name that which they assumed or imagined. Names, in his opinion, ought to express ideas; but there were many names introduced which he thought were used for no better purpose than to express the absence of ideas. It often happened that when exploring any particular part of a field, they got a rational clue which led them clearly and well for a certain way; and they failed to follow it further. The cases were numberless, but one of the most important was that of chemical combination itself. Complex bodies were far more numerous than the few simple bodies with which they had to do, and while in the habit of using the term chemical combination, they had concealed their ignorance of the state of combination. Others used the term molecular combination, and there again they concealed their ignorance of the bodies to which it was applied. Among the present anomalies in names there was one which he ventured to submit to the consideration of the Section, and which had grown up to some extent of late, and that was the replacing of empirical names of things by names, which, while he would call them rational, because they served to recall intelligibly and without ambiguity, served to recall the number of atoms. He mentioned such cases by way of illustrating the practice which had seemed to him to be gaining ground of late years, for the purpose, as some said, of increasing the clearness of statement. He had no doubt that the words were framed for the purpose of conveying to the mind something useful to know, and as names formed on that principle had been found to be based on those superseded by others, he thought when they came to such names as indicated molecular composition it was better to avoid them, because, as he had said, they had not arrived at finality. The chemists of fifty years ago were as confident as chemists of the present day in the matter of nomenclature; and therefore the more they could obtain names without ambiguity and without liability to change in the future, the more probable was it that such names would stand and continue to be used. A crowd of material presented itself just then to his mind, but he did not think it would be well to trouble the Section with further remarks. He merely wished to throw out the ball for his colleagues to deal with.

Cellulose and Coal, by C. F. Cross, B.Sc., and E. J. Bevan. —This is a continuation of the authors' researches on bast fibre (Chem. Soc. *Journ.*, 1880, abstr. 666). By the action of sulphuric acid (sp. gr. 1·65), at 70°, on jute fibre, an insoluble, black, spongy substance has been obtained; that the cellulose

of the fibre contributes to the formation of the substance, is shown by the formation of a similar compound from pure cellulose and dextrin. A chlorinated product ($C_{20}H_{16}Cl_4O_{10}$) has been obtained from this black substance, its properties are similar to those of the aromatic substance described in a previous paper (*loc. cit.*). The production of this spongy substance is usually a destructive one, and attended with an evolution of CO_2 and the production of acetic acid, &c. It is not, however, necessarily so, for when the action of the sulphuric acid is arrested before the evolution of carbonic dioxide, a reddish brown solution is obtained, from which when poured into water a copious flocculent precipitate is obtained, of a body very similar in chemical properties to the black substance described above. The chlorine substitution products are easily converted into astringent bodies, producing dark-coloured precipitates with iron salts and copious coagulation with gelatine. These facts, together with the following :—(*a*) Meissner and Sheppard's conclusion that the hippuric acid of herbivorous urine is derived from an aromatic body present in the fodder, apparently a form of cellulose, which the authors have identified as similar to the characteristic consitituent of bast fibre; (*b*) the previous demonstration by the authors of the homogeneous nature of jute fibre, and that in its resolution the percentage yield of cellulose may be increased apparently at the expense of the aromatic constituent; (*c*) that the process of liquification (or the formation of tannin-like substances) is said by microscopists to be due to an intrinsic modification of the substances of the cell-walls, *i.e.*, of the cellulose, and not to an infiltration of the substances present in the cell cavity; (*d*) the numerous cases in which tannic acid is formed at the expense of plant structures of the nature of cellulose—lead the authors to conclude that, until the contrary is proved, lignin must be regarded as derived from cellulose by chemical modification. The spongy black substance, previously described, dries to a hard mass resembling cannel coal, with which the authors have compared it, and have obtained similar products of chlorination and nitration, and further support of the opinion that coal is not carbonaceous in any more special sense than alcohol, but is rather, as supposed by Balzer, composed of C, O, H, N bodies, which are genetically, if not homologously related. The authors suggest that cellulose, lignite, peat, lignin, and anthracite are terms of an infinite series differentiated under the conditions of their formation.

Hydration of Salts and Acids, by C. F. Cross, B.Sc.—The method adopted by the author for investigating the rate of hydration of a substance consisted in exposing about 1 gramme of the substance in a bell-jar of 2000 c.c. capacity, to an atmosphere saturated with aqueous vapour. After a critical investigation of the probable errors, the "Jolly" Federwaage was used to make the numerous weighings required, and thus the method of observation was rendered very expeditious. The paper contains diagrams representing the velocities of hydration for certain salts and oxides. The author has observed that, under these "artificial" conditions of exposure, all the soluble salts examined deliquesce. This takes place in some cases without previous hydration, *e.g.*, with potassium bichromate, and in such cases the water may be removed by pressure between blotting paper. In other cases, *e.g.*, with $CuSO_4$, the salt deliquesces after uniting with water of chemical hydration, and in a different manner. It would therefore appear that the continuity of the phenomena of hydration and solution, as regards the determining cause, is demonstrated by these observations.

On Colliery Explosions, by W. Galloway.—The author gave an account of his experiments made to show the influence of coal-dust in colliery explosions. In July, 1878, he made three sets of experiments with different kinds of apparatus. In the first set, in which coal-gas was used instead of fire-damp, and the gas and air were carefully measured, and then coal-dust added, it was shown that 2 per cent. of gas, mixed with air, was rendered inflammable when coal-dust was added; 3 per cent. of gas made this mixture slightly explosive; 4 per cent. made it still more explosive; and 5 per cent. produced a violent explosion. The total quantity of gas and air mixture was little more than a cubic foot. In the second set it was shown that the return air of a mine containing 2 per cent. of fire-damp became inflammable when coal-dust was added to it. In the third set the explosion of a mixture of air and fire-damp was made to raise and ignite coal-dust scattered along the floor of an artificial gallery 70 or 80 feet long, and 14 inches square inside. The flame of the fire-damp explosion alone was found to be 7 feet or 8 feet long; the flame of coal-dust in pure air was 35 feet or 40 feet long; and the flame of coal-dust in the return air employed in the first set of experiments was 80 or 90 feet long. The publication of these results called further attention to the subject, and after the Seaham explosion the Home Secretary requested Dr. Abel to inquire, amongst other things, into the influence of coal-dust in promoting that disaster. Prof. Abel made experiments near Wigan, and obtained results similar in kind to the author's, but different in some respects. In July of the present year the author made experiments with apparatus of the following description: A sheet-iron cylinder 6 feet long by 2 feet in diameter, closed at one end and open at the other, had its open end bolted to a wooden gallery 126 feet long by 2 feet square inside. One end of the wooden gallery was thus closed by the sheet iron cylinder, an explosion chamber, and the other end was open. Six sheets of newspaper were placed between this open end of the explosion chamber and this gallery, and a tight joint was ensured by means of screws. Rather less than 2 cubic feet of fire-damp was carefully measured and introduced into the explosion chamber. The wooden gallery contained only pure air. The air and fire-damp contained in the explosion chamber was thoroughly mixed by means of an appropriate mechanical arrangement, and the mixture was exploded. The explosion burst the sheets of paper, and the resulting flame travelled about 12 feet or 14 feet along the gallery, and as suddenly disappeared. The gallery was then strewed with a layer of the coal-dust from ⅛ inch to ¼ inch thick along its floor, and some was placed on shelves which stood in sets of three, one above the other, at distances of 10 feet from each other, along the gallery. The same arrangement as before was then made in regard to preparing for a fire-damp explosion, exactly the same quantity of fire-damp being measured, mixed, and exploded. By this explosion of the fire-damp mixture the coal-dust was raised in a cloud throughout the whole length of the gallery, part of it was projected out into the air to a distance of 20 feet or 30 feet beyond the end, and, after the lapse of an appreciable interval of time, the flame found its way to the end of the gallery and flashed out through the cloud of dust to a greater or less distance according to circumstances. The greatest length of flame thus obtained with coal-dust and pure air was 147 feet on one occasion, and from 100 feet to 140 feet very often. He considered that these results proved in the most convincing manner that coal-dust formed an inflammable mixture with pure air, and they settled once for all the question as to how an explosion in one district of a dry and dusty mine could penetrate to the most distant parts of every other district of the workings in the same mine. In conclusion the author spoke of the necessity of keeping the floors of mines damp, and thus lessening the dangerous influence of coal-dust.

SECTION C—Geology

A preliminary Account of the Working of Dowkerbottom Cave in Craven during August, 1881, by E. B. Poulton, M.A., F.G.S.—Dowkerbottom Cave is 1250 feet above the sea, between Arncliffe and Kilnsey. Its mouth is merely a fall in the roof of the cave, which stretches from either end of the fissure thus formed. The original mouth is not now visible, but is probably to be found at the foot of a slope to the south. During most of its course the chambers and passages of the cave are not separated by any great thickness of rock from the ground above, and thus other falls must be expected to occur. The eastern division of the cave is about 450 feet long, and has three fine chambers separated by two passages, the first very short, and the second very long. This division ends under high ground, and the true mouth must be in the other, or western cave. The last chamber is characterised by mechanical deposits—blocks of limestone fallen from the roof and a stiff brown clay beneath. In the other chambers and passages are chemical deposits—hard and soft stalagmite. The western division is smaller, but also contains three chambers and two passages. It must be about 250 feet long. Chemical deposits, with some falls from the roof, are present throughout. In former workings by Mr. Farrer, Mr. Denny, and Mr. Jackson, the first chambers were explored in their surface layers at least, and here were found the numerous metal and bone ornaments and implements, together with the bones of animals usually found in the historic layers (of Romano-British age) in caves. The second passages have also been worked, and part of the second chamber on the eastern side. Other parts of the cave appear to be quite untouched. The great